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*Document Version*

Publisher's PDF, also known as Version of record

*Publication date:*

2014

[Link to publication in University of Groningen/UMCG research database](#)

*Citation for published version (APA):*

Allers, M., & Vermeulen, W. (2014). *Fiscal equalization, capitalization and the flypaper effect*. (SOM Research Reports; Vol. 14023-EEF). University of Groningen, SOM research school.

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August 28, 2014

We thank Christian Hilber, Coen Teulings and participants of seminars at the Spatial Economics Research Centre, London School of Economics, and at Cambridge University, as well as participants of the 2014 Summer Symposium of the UBC Centre for Urban Economics and Real Estate and the 2014 annual congress of the IIPF for useful comments. We thank the Dutch Association of Realtors (Nederlandse Vereniging van Makelaars o.g. en vastgoeddeskundigen NVM) for providing data on individual housing transactions. The Ministry of Interior Affairs has funded the data collection for this research. Address correspondence to: Wouter Vermeulen, CPB Netherlands Bureau for Economic Policy Analysis, P.O. Box 80510, 2508 GM, The Hague, The Netherlands. Phone: +31 70 3383467. Fax: +31 70 3383350. Email: [w.vermeulen@cpb.nl](mailto:w.vermeulen@cpb.nl).

# Fiscal Equalization, Capitalization and the Flypaper Effect

## **Abstract**

We investigate the impact of equalizing grants to municipalities on local house prices in the Netherlands. Our identification exploits a nonlinear time-pattern in the implementation of a reform of the grant allocation system, which was gradually introduced and carried out in two subsequent stages that targeted different policy domains. Results indicate that the change in grants was fully capitalized. We also find that only a small fraction was passed on to residents through property taxes, consistent with the flypaper effect. Furthermore, we rule out a significant effect on the size of the local bureaucracy. Hence, changes in grants appear to have been largely accommodated through an adjustment of the provision of public services that are valued by the marginal homebuyer. A placebo reform, in which additional funds for municipalities came with a new responsibility for school accommodation, had no significant effect on house prices.

**JEL classification:** H7, H81, R2, R3, R51

**Keywords:** intergovernmental transfers, fiscal equalization, capitalization, flypaper effect

# 1 Introduction

In most countries, local governments are partly funded by central government grants and in many cases, the grant allocation system takes account of disparities in either spending needs, fiscal capacity or both.<sup>1</sup> This paper provides new empirical evidence on the capitalization of equalizing grants into local house prices. We consider a reform of the allocation system of unconditional general grants to municipalities in the Netherlands, which increased the weight of socioeconomic characteristics, such as poverty rates and the share of minorities, and introduced partial equalization of tax capacity. Reform-induced changes in annual grants varied considerably over municipalities, with a standard deviation of 56 euro per inhabitant. Such changes are expected to capitalize when they are accommodated through an adjustment of public service levels or tax rates, which affect the local willingness to pay for housing.

Our identification exploits a non-linearity in the time pattern of reform-induced changes in grants. In order to allow municipalities to adjust their policies, transition grants were used to cushion changes and the reform was carried out in two stages, starting in 1997 and 2001, respectively, that targeted different categories of variables in the allocation system. With data on grants from 1992 to 2010 and a municipal house price index from 1995 to 2010, the resulting nonlinear time pattern allows us to control for any municipality-specific linear trends in unobserved variables that may correlate with grants and house prices.

Estimation results indicate full capitalization at a real discount rate of 3 per cent, a result which is robust to identifying on either of the two stages of the reform separately. We further validate our identification strategy by considering a placebo reform, which made municipalities responsible for school buildings and provided the corresponding resources. As expected, changes in grants induced by this reform do not significantly affect house prices because, in contrast to the general grant reform, in this case the extra money came with new spending responsibilities and could not be spent freely on improving public services or lowering tax rates.

In order to shed light on the channel through which changes in grants have induced changes in house prices, we investigate the extent to which municipalities have accommodated these changes through an adjustment of local taxes. Conventional economic theory predicts that changes in grants will be passed on to residents almost entirely through

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<sup>1</sup> For example, fiscal equalization is common in Europe and it has a long tradition in Canada, Australia and some developing countries like India. While equalizing grants from the federal to state governments are limited in the US, many states provide equalizing grants to local jurisdictions – notably school districts.

this channel, as the marginal propensity to spend on local public services should be the same for grants and for private income, for which 10 per cent is a plausible upper estimate.<sup>2</sup> However, we find that property tax revenue, which is the only relevant source of local tax revenue in the Netherlands, falls with only about 16 cents for each euro of additional grants. The propensity for local governments to spend a large share of grants is known in the economic literature as the *flypaper effect*.<sup>3</sup> As far we know, our paper is the first to relate this well-documented phenomenon to house price capitalization.

Capitalization indicates how people value the way in which municipalities spend their money and in this respect, it offers a new perspective on the flypaper effect. While at odds with conventional economic theory, this effect would appear to be less of an anomaly if rather than passing them on through local taxes, local governments choose to spend changes in grants in a way that is largely valued by local residents. Under admittedly stringent assumptions, full capitalization implies that the level of local public service provision is efficient, as the marginal euro of grants is converted into one euro of value for residents – consistent with the well-known Samuelson condition.<sup>4</sup> While this result does not hold up in a sorting framework with heterogeneous tastes for local public goods and for locations, capitalization in such a framework still indicates that at least the marginal homebuyer attaches considerable value to how the marginal euro of grants is spent.<sup>5</sup>

It follows that capitalization is difficult to reconcile with a particular class of explanations for the flypaper effect that are based on local government failure. In this strand of the literature, Wyckoff (1988) demonstrates the existence of a ‘bureaucratic flypaper effect’ under the assumptions that local bureaucrats are interested in maximizing the budget of their departments rather than serving the public interest and that voters are uninformed about the amount of grants that local governments receive (see also Filimon *et al.*, 1982). In a more recent paper, Brollo *et al.* (2013) propose a model in which additional grants allow self-interested politicians to extract more rents from uninformed voters. Moreover, using Brazilian data, these authors also find empirically that more grants lead to more corruption and reduced quality of local politicians. Such effects of additional grants are unlikely to make local

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<sup>2</sup> Following Inman (2008), this assumes that the income elasticity of demand for local public services is smaller than one. Municipal spending does not exceed 10 per cent of GDP in the Netherlands.

<sup>3</sup> Hines and Thaler (1995) and Inman (2008) survey the literature and discuss possible explanations. See e.g. Dahlberg *et al.* (2008), Lutz (2010), Dahlby (2011) or Lundqvist (2013) for recent contributions.

<sup>4</sup> See Brueckner (1979, 1982). Barrow and Rouse (2004) adapt this theoretical framework to equalizing grants.

<sup>5</sup> See for instance the sorting model in Bayer *et al.* (2007). Hilber (2011) surveys the recent literature on house price capitalization and local public sector efficiency, emphasizing the role of supply constraints in this framework (see also Hilber and Vermeulen, 2014). The low elasticity of housing supply in the Netherlands (Vermeulen and Rouwendal, 2007) may thus further explain the high capitalization rate found in this paper.



communities more attractive to potential homebuyers, so we should expect house prices to be largely unaffected.

As an alternative test of the presence of a bureaucratic flypaper effect in the Netherlands, we explore the impact of the reform-induced change in grants on the number of municipal civil servants in full-time equivalents. Our estimate is not statistically significant, but it is sufficiently precise to rule out an economically meaningful effect.<sup>6</sup> Thus, by and large, additional grants appear not to have been used to expand local bureaucracies.

The implications of capitalization extend beyond the valuation of local public services. In particular, it may jeopardize distributional objectives of fiscal equalization, which are generally viewed as an important justification (Oates, 1999). The reform that identifies our capitalization result benefitted low-income municipalities and our results suggest that local public services in these municipalities have improved as a result. However, residents who bought a house after the reform and renters in the private sector have paid the price in terms of higher housing costs, implying that for them the increase in public consumption has come at the expense of reduced private consumption. Renters in the social sector, which constitutes about a third of the total housing stock in the Netherlands, have gained only to the extent that housing associations have not passed on the change in housing value. The main beneficiaries, though, were homeowners and landlords prior to the reform, who were typically better off than the average citizen. The concluding section will elaborate on implications for policy.

In spite of a rich empirical literature on the capitalization of fiscal differentials into house prices, evidence on the capitalization of intergovernmental transfers is relatively scarce.<sup>7</sup> Hilber *et al.* (2011) find evidence of substantial to full capitalization in a study of central government grants to local authorities in England. In their analysis, electoral targeting of grants by the incumbent Labour party provides the source of exogenous variation. In contrast, we identify on a reform that was designed to make the grant system more equitable, so that our analysis provides direct evidence on the impact of fiscal equalization. Moreover, the variation in reform-induced changes in grants in our analysis is considerably larger, enabling stronger and more robust identification.<sup>8</sup>

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<sup>6</sup> Using Swedish data, Lundqvist *et al.* (2014) also find that general grants do not stimulate employment in the local public sector in general, although they do find a positive and statistically significant effect on administrative personnel.

<sup>7</sup> The capitalization literature is surveyed in Chaudry-Shah (1988), Ross and Yinger (1999) and Hilber (2011).

<sup>8</sup> Identification in Hilber *et al.* (2011) is based on the positive but diminishing impact of Labour dominance on grants, after controlling for the linear effect of Labour's share of seats. An increase in Labour's share from 40 per cent to 50 per cent is found to reduce grants per capita by 13 pounds (year 2008), corresponding to 16 euros (year 2010). This figure may be contrasted to the standard deviation of reform-induced changes in annual grants of 56 euro per inhabitant in our analysis.

Identifying on reforms of state school financing formulas, which have become more equalizing in many US states over their period of observation, Barrow and Rouse (2004) find evidence of full capitalization of state education aid to school districts. They instrument the actual change in state aid with the change in aid that each district would have received on the basis of the post-reform formulas and pre-reform characteristics. The validity of this approach relies on the assumption that changes in district housing values are not correlated with the pre-reform district characteristics that are used to construct the instrument through other channels than capitalization. Our identification requires less restrictive assumptions in this respect, as we control for arbitrary linear time trends in grants and house prices.

The contributions of this paper may thus be summarized as follows. In the first place, our identification of the capitalization of equalizing grants into house prices is more direct and robust than in the two earlier papers on this topic. In the second place, we provide new evidence on the channel, thus relating our capitalization result to a longstanding literature on the flypaper effect. In the third place, our findings challenge explanations of this effect that are based on local government failure.

## **2 Institutional setting and reform of grant allocation**

### **2.1 Local government in the Netherlands**

The Netherlands is a highly centralized country. Even though many governmental tasks have been assigned to municipalities, national regulation or guidelines ensure that basic public services are to a large extent uniform across the country.<sup>9</sup> For instance, municipalities administer welfare, yet the norms for assignment and benefit levels are set nationally (Toolsema and Allers, 2014). About a third of the budget of municipalities is spent on welfare and social services. Other important tasks concern spatial planning, urban renewal, local infrastructure, waste collection and disposal, health care and cultural and recreational facilities. In contrast with many other countries, municipal responsibilities in the domain of education are mainly limited to the construction and upkeep of school buildings.

A large proportion of these tasks benefits poorer households disproportionately. It is a well-known tenet of the classical theory of fiscal federalism that policies with a redistributive nature cannot be financed by local taxes without distorting the location choice of households

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<sup>9</sup> Allers (2011) and Bos (2012) describe task assignment and local government finance in the Netherlands.

(see e.g., Buchanan, 1950, or Boadway and Flatters, 1982). This institutional setup thus inherently requires some form of fiscal equalization.

Fiscal equalization in the Netherlands is based on the principle that each municipality should be able to provide the same level of services at the same property tax rate. It reflects the considerable popular resistance to nonmarginal differences in local public services and taxes in this country, arguably related to its small size and social homogeneity (Goedhart, 1973). Unconditional general grants, which constitute more than a third of municipal revenue on average, are allocated on the basis of extended formulas that take account of local needs through a wide range of variables. Equalization of local tax capacity was introduced in the 1997 grant reform.

Differences across municipalities are further diminished as central government grants constitute the main source of municipal revenue. Apart from an equalizing unconditional grant, municipalities receive many different conditional grants to finance mandated tasks. The share of local expenditure covered by local taxes does not exceed 10 per cent on average, which is exceptionally low from an international perspective (Blöchliger and King, 2006). The only important local taxes are property taxes. Some regulation of the amount by which municipalities can raise their property tax revenue has been introduced in 2006.<sup>10</sup> The marginal role of property taxes also implies that a small increase in municipal income through this channel requires a large relative increase in the property tax rate. In order to increase local spending by one per cent, the property tax rate must be raised by 5 – 42 percent, depending on the municipality (Allers *et al.*, 2010). The political cost of such an increase is likely to be considerable.<sup>11</sup> Hence, in practice, municipalities have little discretion about their income.

The combination of limited income discretion and a dominant role of centrally assigned tasks may partly explain the high capitalization rate found in this paper. If the amount of funds that municipalities can freely spend according to their own preferences is limited, it is conceivable that a marginal increase in general grants that does not come with additional tasks will be spent on services that are efficiently or even underprovided. Hence, this could induce even more than full capitalization (Brueckner, 1979; 1982).

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<sup>10</sup> In 2006 and 2007, property tax rates were capped at the municipality level. In 2008, the central government introduced a ceiling on the total rise in property tax revenues of all municipalities, which is often binding, although individual municipalities can raise property taxes as much as they like.

<sup>11</sup> Allers and Elhorst (2005) provide evidence of yardstick competition on property tax rates in the Netherlands. The political costs are likely to be enhanced by the salience of the property tax (Cabral and Hoxby, 2012).

## 2.2 The reform of general grant allocation

In 1997, a new system for the allocation of the (unconditional) general grant was introduced that changed the measurement of spending needs, equalized the local property tax base and, most importantly, adjusted the weights accorded to different types of spending needs.

The preceding grant allocation system measured needs on the basis of regression analysis of municipal spending, in a similar way as in various other countries (see e.g., Ladd, 1994). However, the reverse impact of grant revenue on spending levels makes this approach problematic, particularly in the Netherlands where municipalities rely for a considerable part of their revenues on central government grants.<sup>12</sup> Hence, the new method was partly based on judgement of acceptable spending levels instead – Boerboom and Huigslout (2008) provide a detailed description.

The 1997 reform introduced the base of the property tax, which constitutes the main source of local tax revenue, as a new variable in the grant allocation formula. Note that it is not actual tax revenue that is equalized but the tax base, and that this base is equalized only partially.<sup>13</sup> Notwithstanding this reform, spending needs still dominate the allocation formula of central government grants, as local tax revenue covers only a small part of municipal expenditure.

When it was introduced, the new grant allocation formula consisted of about 50 variables. It put less weight than its predecessor on municipality size, while strengthening the equalization of spending needs relating to disparities in socioeconomic characteristics (poverty, minorities), physical characteristics (soil structure) and spillovers to adjacent municipalities. These changes were introduced in two stages. About two thirds of the re-allocation of general grants took place in the 1997 stage and the remaining part of the revision came into effect in the 2001 stage. While the first stage focused on measures for socioeconomic composition and the burdens on central cities, the second stage covered mainly physical characteristics.

Summary statistics for the permanent changes in grants due to these two stages of the reform, divided by the population in 1997, are reported in Table 1. The redistributive nature of the reform is illustrated in Figure 1, which scatters the per capita change in grants resulting from both reforms together against personal income in 2001. The fitted trend in this figure

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<sup>12</sup> This ‘circularity problem’ has been noticed in the UK as well (Hall *et al.*, 1996).

<sup>13</sup> During 1997-2000, property value assessment was not yet uniform and municipalities still used different base years. In 2001-2004, every municipality used 1999 as a base year; in 2005 and 2006, the base year was 2003, and in 2007, the base year was 2005. Since 2008, property values are assessed annually, the base year being the year before the fiscal year.

indicates that, on average, municipalities in which per capita income was 10% higher saw their annual grant reduced by almost 40 euro per inhabitant. Correlations with personal income in 2001 are reported for each stage separately in Table 2. Given the differences in focus, it is not surprising that the 1997 stage shows a particularly strong negative correlation with income, whereas the correlation of the 2001 stage with income turns out to be positive. The 1997 stage dominates the aggregate effect as this reform was larger.

A scatter of the 2001 stage-induced change in grants against the 1997 stage-induced change is shown in Figure 2. As seen in this figure and in Table 2, the two stages are weakly negatively correlated. By implication, we can meaningfully test the robustness of our identification strategy by constructing instruments on the basis of each stage separately.

The reform was introduced gradually in order to allow municipalities to adjust their policies. While the new formulas came into effect immediately, resulting changes in grants to individual municipalities were smoothed out over five year periods using transition grants (BZK, 1996). In the first stage (1997-2001), these transition grants ensured that a municipality's annual grant change resulting from the revision of the allocation system was maximized at 5 per cent. In the fourth and last year of the transition grant, municipalities where the grant changed more than 25 per cent received a redemption grant to buy off future transition grants.<sup>14</sup> Municipalities which gained as a result of the grant revision received negative transition grants. A similar transition applied in the second stage (2001-2005).<sup>15</sup>

As discussed in more detail in the next section, our identification of the causal impact of exogenous changes in general grants uses the variables 'Reform of 1997' and 'Reform of 2001', defined as the sums of permanent changes due to both stages of the overall reform and the corresponding transition grants. Figure 3 illustrates the permanent changes and the variables Reform of 1997 and Reform of 2001 for the municipality of Amsterdam. Furthermore, we define the variable 'Reforms of 1997 and 2001 jointly' as the sum of Reform of 1997 and Reform of 2001. In order to avoid simultaneity bias, these variables are all scaled to the population in 1997 rather than to contemporaneous population. Summary statistics are again reported in Table 1. Figure 4 shows Reforms of 1997 and 2001 jointly for the four municipalities that received the largest increase or decrease in general grants in either the first

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<sup>14</sup> For instance, a municipality loosing 25 per cent of its grant in the first stage received a transition grant of 20 per cent of the permanent change in 1997, of 15 per cent in 1998, of 10 per cent in 1999 and of 5 per cent in 2000. A municipality that saw its grant reduced by 40 per cent would be eligible for a positive transition grant during 7 years. In the fourth year, the remaining instalments were bought off.

<sup>15</sup> In the second stage, supplementary grants ensured that grant changes became effective in five annual steps, of 10, 15, 25, 25 and 25 per cent, respectively (BZK, 2000). Municipalities losing more than one hundred guilders (45 euro) per capita received a redemption grant in the fourth year, in a similar way as in stage 1.

or the second stage – excluding the 24 out of 419 municipalities in our sample for which the time pattern of the reform-induced change in grants was atypical because of a redemption grant. Figure 5 shows Reform of 1997 for the municipality of Bloemendaal, which received the largest redemption grant.

### **2.3 Placebo: decentralization of school accommodation**

We use the decentralization of school accommodation in 1997 to validate our identification strategy. This reform changed the way of funding a service that was already provided. School buildings for children under 12 used to be financed by the central government, which provided funds to the school boards and organisations that built and maintained schools. This task was decentralized to municipalities in 1997. In order to provide them with the necessary means, the general grant was raised by the amount spent previously by the central government, minus an ‘efficiency deduction’ of about 7 per cent. As the general grant is non-earmarked, municipalities were free to spend the extra funds as they liked, but they had to take over the task of providing adequate school buildings. Therefore, most of the additional money could not be spent on an improvement of public services or a reduction of taxes, so we would not expect to find substantial capitalization.

The extra funds were allocated to municipalities according to a formula derived in a way similar to the new general grant allocation system that was introduced in 1997 and 2001 (see above), of which it now forms a part.<sup>16</sup> The new funding system was introduced gradually using transition grants, in a way not unlike the gradual introduction of the new general grant allocation system described above.<sup>17</sup> Transition grants included some minor additional grants relating to school accommodation and sport parks that could not be separated out. Table 1 reports summary statistics for the permanent change in grants due to the decentralization of school accommodation, for the transition grant and for the sum of these

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<sup>16</sup> Some of the variables of the school buildings formula, like inhabitants younger than 20 and minorities, were introduced simultaneously with the general grant reform. It is possible, however, to split the weights of these variables into the part which is related to school buildings and the part which is not.

<sup>17</sup> To this end, the funds previously spent by the central government within the territory of each municipality were calculated and subtracted from the new budget per municipality. If the result was negative, the municipality received a transition grant that capped the annual reduction to 0.75 per cent of the amount previously spent within the municipality. This transition grant was paid out during a maximum of 5 years (1997 up to 2001). For instance, if a municipality received 2.5 per cent less than was previously spent within its territory, it received, in 1997, a supplementary grant of 1.75 per cent of the amount previously spent; in 1998 it received a supplementary grant of 1.0 per cent of this amount, in 1999 of 0.25 per cent of this amount and in 2000 and 2001 nothing. If the reduction in funds for school buildings exceeded 3.75 per cent, the redemption grant was spread out over the entire period. For technical reasons, municipalities where school building budgets increased could not receive a negative supplementary grant. To avoid negative supplementary grants, every municipality received a positive ‘base grant’, to which the rest of the supplementary grant (positive or negative) was added. This base grant was financed by temporarily (1997-2001) reducing the main budget of the general grant.

variables, which we define as the ‘Reform of financing school buildings’. Again, these variables are all scaled to the population in 1997.

### 3 Empirical analysis

#### 3.1 Specification and identification strategy

The impact of general grants on house prices is estimated using the model

$$\log(P_{i,t}) = \alpha_i + \beta_t + \gamma_i t + \delta G_{i,t-2} + \varepsilon_{i,t}, \quad (1)$$

where  $P_{i,t}$  is a hedonic house price index for municipality  $i$  in year  $t$ ,  $\alpha$  is a municipality fixed effect,  $\beta$  is a year fixed effect,  $t$  is a linear time trend that is allowed a municipality-specific effect,  $G_{i,t-2}$  is the general grant per capita lagged by two years and  $\varepsilon_{i,t}$  is an error term. The dependent variable will be replaced by local property tax revenue and municipal staff per capita in subsequent analyses, as discussed in subsections 3.4 and 3.5 respectively.

Following Hilber *et al.* (2011), we use a semi-log specification for the estimation of house price capitalization. Theory predicts a linear relationship (see e.g., Barrow and Rouse, 2004), yet least-squares estimates of a linear model are more sensitive to outliers. Moreover, the municipality and year fixed effects capture unobserved heterogeneity more accurately in a semi-log model. For instance, changes in macro-economic variables like the interest rate that have proportionate rather than additive effects on house prices are not well controlled for in a linear specification. We show below that the estimation of a linear model yields very similar capitalization rates, once proportionate municipality and year fixed effects are removed from the house price index.

Estimation of the model in Equation (1) by ordinary least squares may yield biased results for various reasons. For instance, if grants increase because the central government devolves a task to municipalities, something that occurred frequently in our research period, house prices are likely to be unaffected if the additional funds just cover additional expenses. In that case, the unobserved change in tasks that comes with the change in grants creates an omitted variables bias. It is also possible that changes in the socioeconomic composition of a municipality affect both grants and house prices. For instance, a rise in the share of wealthy households will reduce the amount of grants through the fiscal equalization scheme, while

there may also be an independent effect on house prices.<sup>18</sup> Furthermore, the reform may be targeted at municipalities with low or decreasing house prices, which would also induce systematic correlation with local house price dynamics through some other channel than capitalization.

Our strategy for dealing with endogeneity of per capita general grants consists of two main ingredients. In the first place, we instrument this variable with the change in grants induced by the reform of the general grant allocation – i.e. the variable Reforms of 1997 and 2001 jointly as defined in the previous section. Identification on changes in grants due to this reform does not lead to bias from omitted tasks, because it left tasks unaffected. The reform also predates dynamics in socioeconomic composition that are contemporaneous with the dependent variable, because changes in grants were already fixed before it was implemented.<sup>19</sup> Hence, contemporaneous changes in the socioeconomic composition of a municipality cannot drive both the reform and changes in house prices. However, contemporaneous changes in socioeconomic composition may still be driven by reform-induced changes in grants, so that variables that correlate with socioeconomic composition are ‘bad controls’ (Angrist and Pischke, 2009).<sup>20</sup> We therefore exclude socioeconomic or demographic control variables from our specification.

The inclusion of municipality-specific linear time trends in our model constitutes the second main ingredient of our identification strategy. Municipality fixed effects control for any targeting of the reform on time-invariant characteristics of municipalities that correlate with house prices levels. However, the reform may also favour municipalities in which house prices trend downwards, possibly as a result of deteriorating socioeconomic conditions. As Figures 3 and 4 illustrate, the reform-induced change in grants varies nonlinearly over time. Our estimates of capitalization may therefore be identified on deviations of this pattern from a linear time trend. The exclusion restriction is that after controlling for municipality and year fixed effects, such deviations do not correlate systematically with house price deviations from a linear time trend through other channels than capitalization. While one could imagine the

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<sup>18</sup> House prices may rise if people have a preference for living near wealthy or highly educated households, or if the variety of amenities such as shops, restaurants and cultural facilities on offer in an area depends on the income level of its residents (see e.g. Brueckner *et al.*, 1999).

<sup>19</sup> Transition grants were based on municipal characteristics in the last year before each of the two stages of the grant revision came into effect. As a result, they reflect only changes in allocation formulas, not changes in local characteristics. Furthermore, the first stage of the revision was based on an analysis of realized spending in 1990 and budgeted spending for 1992, while the second stage was based on an analysis of realized spending in 1995 and budgeted spending for 1997 (Van Zaalen, 2002).

<sup>20</sup> For example, municipalities may invest additional grants in local amenities that facilitate gentrification, thus altering their age and income composition.



general grant reform to target municipalities on a downward trajectory, it seems unlikely that policymakers targeted on predicted nonlinearities in this trajectory.

We validate our exclusion restriction by identifying on each of the two stages in which the reform was implemented separately and by identifying on the change in grants due to decentralization of school accommodation. If correlation of the reform with subsequent house price dynamics through omitted variables were an issue even after controlling for arbitrary linear time trends, it seems unlikely that this would affect each of the two weakly correlated stages in the same way. Hence, validity of our exclusion restriction is supported if identifying on either Reform of 1997 or Reform of 2001 yields similar results. Identification on the placebo Reform of financing school buildings instrument should not yield substantial capitalization, as this reform came with additional spending obligations.

Our identification strategy does not account for the fact that some systematic correlation of the reform of general grant allocation with subsequent house prices may occur through tax base equalization. If a reform-induced rise in grants capitalizes into house prices, then partial equalization of the property tax base will lower subsequent general grants. This would induce a downward bias in our estimate of the capitalization rate. Quantitatively, however, the implied bias is small, because of the limited role of property taxation. Under the assumption of full capitalization, it never exceeds 6 per cent over our period of observation.<sup>21</sup>

Finally, our identification strategy would not work out in a world with full information and perfect foresight. Forward looking behaviour implies that any new information on the reform should have capitalized as soon as it became publicly available. Since the structure of the reform was already by and large decided on prior to 1995, this would mean that most of the capitalization had already occurred before our period of observation. To the extent that information on the reform did indeed capitalize when it was announced, our estimate of the capitalization rate is conservative. However, we conjecture that most citizens only became aware of the reform in general grant allocation and the way in which it affected their own municipality, when its effect was felt through a change in public service levels or in property taxes. The public outcry when Wassenaar, a municipality that experienced one of the largest reductions in general grants, raised property taxes sharply in 2003 is exemplary in this respect: in a world with full information and perfect foresight, this outcry should have

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<sup>21</sup> In early years, the equalized property tax base was not adjusted to contemporaneous house prices (see footnote 13), so that capitalization of general grants could not give rise to bias. Furthermore, there is some variation over time in the rate at which the property tax base is equalized.

occurred at least seven years earlier.<sup>22</sup> Furthermore, we find that capitalization effects are strongest two years after the reform-induced change in grants has occurred, which seems a plausible time lag for adjusting public service levels and property tax rates.

### 3.2 Data

We estimate equation (1) for the period 1995-2010. A number of municipal amalgamations occurred during this period, so we aggregate all variables in our analysis to the 2010 classification of municipalities. The five northern island municipalities are excluded and we merge a few other very small municipalities in our data, because the number of observed housing transactions would otherwise be too small to estimate a meaningful house price index. This leaves us with a sample of 419 municipalities out of a total of 430.

The house price index is estimated on all housing transactions over our period of observation that were conducted by members of the Dutch Association of Realtors (NVM), which covers the majority of all owner-occupied housing transactions in the Netherlands. Transactions of both single family units and apartments for permanent residence are considered, while dwellings on land lease are excluded. We impute dwellings as not being on leased land if this information is missing and if less than 5 per cent of all dwellings in the municipality are on leased land.<sup>23</sup> This leaves a sample of 1,614,735 observations, or 241 transactions per municipality/year combination on average. Table 1 provides summary statistics on the number of observed housing transactions per municipality and year. The log of the transaction price in 2010 euros is regressed on a large number of dwelling characteristics and on  $16 \times 419$  municipality/year-specific fixed effects.<sup>24</sup> The price index is then constructed from these fixed effects and scaled in such a way that its transactions-weighted average corresponds to the average price in our sample of housing transactions.

General grants are observed for the period 1992-2010. The source of all our data for general grants, reforms and transition grants is the Ministry of Interior Affairs, which has computed the permanent changes in grants by subtracting grant allocation according to the old formula from the allocation according to the new formula (BZK, 1997, 2000). General grants and transition grant amounts are inflated to 2010 euros for each year. Permanent changes are

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<sup>22</sup> Wassenaar managed to put off adapting to the lower grant until 2003 by consuming its substantial financial reserves.

<sup>23</sup> It is verified in a robustness analysis that leaving out the imputed observations does not change our main result.

<sup>24</sup> The dwelling characteristics include plot size (for single family units), size of the dwelling, volume, number of rooms, kitchens and bathrooms, number of floors, dwelling type, period of construction, availability of a balcony or garden, parking space, quality of maintenance and location. Attribute effects are differentiated for single family units and apartments whenever relevant.

inflated to 2010 euros in the year in which they are introduced. Subsequent years are not deflated relative to this base year because grant amounts grow annually in tandem with central government spending, which takes account of inflation.

The analysis of local property tax revenue per capita uses total residential property tax revenue from 1997 onwards, calculated as tax base (obtained from Statistics Netherlands) times tax rate (obtained from COELO). This variable sums revenues from a tax on owners and a tax on users of residential property, the latter of which was abolished in 2006. The analysis of municipal staff per 1000 inhabitants uses staff employed by municipalities in full time equivalents, which is available from 1998 onwards from the Ministry of Interior Affairs. Information on the number of inhabitants per municipality is provided by Statistics Netherlands. Finally, personal income in 2001, which is used for descriptive purposes only, is obtained also from Statistics Netherlands. It refers to the average disposable income for persons who had a job throughout the year.

### **3.3 Results on capitalization**

Table 3 contains our baseline results. In this table and in the remainder of the paper, reported standard errors are clustered at the municipality level. As seen in the first column of Panel A, estimating the model in Equation (1) with OLS while excluding linear time trends yields a weakly significant positive association between the house price index and general grants per capita. Entering linear trends provides a first pass at controlling for the unobserved heterogeneity that may bias this estimate. The second column shows that doing so indeed both raises the coefficient and reduces the standard error of the estimate. However, these linear trends may still be a poor control for omitted tasks to the extent that variation in grants was driven by decentralization of tasks or by changes in the socioeconomic composition of municipalities. These potential sources of bias are removed by instrumenting grants with the reform. Columns (3) and (4) show that this raises the coefficient considerably, consistent with our expectations. Results turn out to be hardly sensitive to the inclusion of linear time trends, however, indicating that the reform is not systematically correlated with trends in house prices through other channels than capitalization. The Kleibergen-Paap statistic indicates that our instrument is strong.

First stage results are shown in Panel B of Table 3. An additional euro of reform-induced change in grants corresponds to about one additional euro in general grants, although the difference from 1 is statistically significant for the estimate in column (4). A large deviation would have been worrisome, as it would imply that part of the variation in general

grants came from other changes that correlate with the reform and that are potentially endogenous (see e.g. Lundqvist, 2013).

Quantitatively, the coefficient in column (4) of Table 3, which is our preferred specification, implies that a 1 euro rise in grants per capita increases house prices by 0.0333 per cent. At a weighted average transaction price of 235,850 euro, this corresponds to an increase of about 78 euro. Figure 6 shows the implied reform-induced house price change per municipality. Municipalities at the 90th percentile of the distribution of reform-induced changes in grants gained 4,273 euro per dwelling, while losses amounted to 5,916 euro per dwelling at the 10th percentile of this distribution. Using a real discount rate of 3 per cent, the present value of a 1 euro rise in annual grants equals 77 euro for an average household of 2.3 persons, implying full capitalization of central government grants.<sup>25</sup> In the 95 per cent confidence interval surrounding this estimate, capitalization rates would be at most 29 per cent higher or lower. Furthermore, using a discount rate of 4 per cent would raise the capitalization rate with about 33 per cent.

Results of identifying grants on either of the two reforms are reported in Table 4. Both instruments still yield strong identification. Estimates from the specifications that include linear time trends are statistically significant and of similar magnitude as our preferred estimate in column (4) of Table 3. More specifically, neither specification rejects our preferred estimate at a significance level of 5 per cent. Nevertheless, the negative and insignificant estimate of grants identified on the 2001 stage when linear trends are excluded from the model indicates systematic correlation between this stage and trends in house prices through omitted variables, while this does not appear to be an issue for the larger part of the reform that took place in 1997. Note also that the first stage coefficients deviate substantially from 1 for the 2001 reform. While the magnitude of this deviation is larger for the specification that includes trends, it is estimated much more precisely for the specification without trends. Thus the 2001 reform appears to be correlated with trends and possibly also deviations from trends in other changes in grants that are potentially endogenous.

Our placebo analysis is reported in Table 5. The reform of financing school buildings identifies changes in grants with sufficient strength, yet it does not yield a statistically significant impact of grants on house prices. Our preferred estimate is rejected at a 1 per cent level of significance for the specification that includes linear time trends. These results are

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<sup>25</sup> Over the period 1995-2010, the real rate of return on Dutch government bonds with a ten year maturity was about 2.5 per cent on average. Uncertainty about future changes in the allocation of grants may warrant a considerable risk premium on top of this rate, so the 3 per cent discount rate is likely conservative.

consistent with our prior expectation that additional grants should not capitalize in house prices to the extent that they come with additional tasks. Note also that the first stage coefficient for the specification that includes linear trends is close to 1, although this does not hold for the specification without trends.

Appendix Tables 1 to 5 report various alternative robustness checks that overall support our main results. In the first of these tables, we estimate a linear model instead of a semi-log model. Proportionate municipality and year fixed effects are removed from the house price index first by regressing the logarithm of this index on municipality and year fixed effects. The residual is then transformed to levels in such a way its transactions-weighted average corresponds with the average price in our sample of housing transactions. Appendix Table 1 reports estimates of variants of Equation (1) in which this index replaces the dependent variable. Results are almost identical to the baseline results in Table 3. In particular, the estimate for our preferred specification indicates that a 1 euro rise in grants per capita increases house prices by about 78 euro.

Experiments with alternative time lags are shown in Appendix Table 2. Consistent with our expectation that capitalization occurs only when public service levels or tax rates have been adjusted, the capitalization effect is strongest for a 2 year time lag.

Appendix Table 3 explores how our results are affected by the peaks in the reform-induced change in grants for municipalities that received a redemption grant. In the first two columns, we estimate baseline specifications for the subsample of municipalities that did not receive redemption grants. Alternatively, we show results for a specification in which the redemption grant is removed from our instrument in columns (3) and (4), or in which it is smoothed out over subsequent years in such a way that municipality's reform-induced annual grant change is maximized at 5 per cent in columns (5) and (6). The last two variants are illustrated for the municipality of Bloemendaal in Figure 5. Overall, it appears that estimates are somewhat higher than our baseline estimate, particularly for the specifications that include linear time trends, for which the first-stage coefficients are also larger than 1.

Since the transition grant for the reform of school finances included some minor additional grants relating to school accommodation and sport parks that could not be separated out, we verify in Appendix Table 4 that grants do not capitalize in house prices if we identify them on the reform excluding transition grants. While the transition grant for the reform of school buildings does not formally belong to the general grant, we have added it to the general grant throughout the analysis in order to be able deal with the reform of school

finances in a consistent way. In this table, however, we exclude the transition grant from the general grant as well.

Finally, Appendix Table 5 contains a host of alternative robustness checks. The first column of this table shows results for a hedonic price index that was estimated on the 1,337,728 transactions for which land lease status was not imputed. The second column verifies that our baseline result is unaffected by including the transition grant for the school accommodation reform in the general grant. The four largest municipalities of Amsterdam, Rotterdam, The Hague and Utrecht are left out of the sample in column (3), as these municipalities negotiate separately over grants with the central government. Municipalities that received a bailout at some point during our sample period are left out in column (4). Column (5) verifies robustness to leaving out observations for which the house price index was based on no more than 20 observations. Finally, in columns (6) and (7), observations are weighted with the average number of housing transactions per municipality and the number of inhabitants in 1997, respectively. All specifications yield statistically significant estimates and none of them rejects our preferred estimate at a significance level of 5 per cent.

### **3.4 Results on the flypaper effect**

The impact of general grants on local taxes is estimated with a variant of equation (1), in which the logarithm of house prices is replaced with total residential property tax revenue per capita, which is available from 1997 onwards. We use the same identification strategy. The corresponding exclusion restriction is that after controlling for municipality and year fixed effects, any deviations of the reform-induced change in grants from a linear time trend do not correlate systematically with deviations of local property tax revenue from a linear time trend through other channels.

Table 6 reports estimation results in the same format as our baseline results in Table 3. Grants are found to affect residential property tax revenue negatively and estimates are statistically significant in all specifications. Identification on the reform raises the coefficient, while inclusion of trends reduces it somewhat. First-stage coefficients are close to 1. In our preferred specification in Column 4, a 1 euro rise in grants per capita reduces property tax revenue per capita by 16 cents, so that more than 80 cents of this euro are not passed on to residents through lower taxes, consistent with the flypaper effect.

Appendix Table 6 repeats the validation tests of Tables 4 and 5. Columns 1 and 2 show results identified on the 1997 stage of the reform, while estimates in Columns 3 and 4 are identified on the 2001 stage. Neither of the specifications that include time trends rejects

our preferred estimate at a significance level of 5 per cent. The impact of grants is identified on the placebo reform of financing school buildings in Columns 5 and 6. As expected, the estimate is close to zero in the specification that includes trends. Curiously, it is significantly negative in the specification without trends. However, the first stage coefficient deviates substantially from 1 for this specification, so it may be biased because of correlation of the 2001 reform with trends in other changes in grants that are endogenous and for which the specification in Column 6 controls.

### **3.5 Results on municipal staff**

For municipal staff per 1000 inhabitants, available from 1998 onwards, we use the same approach as for residential property tax revenue, implying a similar exclusion restriction. Results are shown in Table 7. The impact of grants on municipal staff is statistically insignificant in all specifications except the specification that is identified on the reform and excludes trends, in which the estimated effect is negative. Validation tests reported in Appendix Table 7 are passed as, like in our preferred estimate in Column 4 of Table 7, all coefficients except for the specification identified on the 2001 reform excluding trends are statistically insignificant.

Nevertheless, our preferred estimate is sufficiently precise to rule out a substantial effect of grants on municipal staff. The largest effect in the 95 per cent confidence interval is 1.58, meaning that 1000 euros of additional grants per capita would raise municipal staff with 1.58 FTEs per 1000 inhabitants. Even if a civil servant would cost 60,000 euros per year, which is a generous upper bound, then expenditure on municipal staff would still amount to only 10 cents per euro.

This finding may raise the question how municipalities that see their grants increased can improve public services without hiring more staff. A first answer is that the money may be spent on capital rather than labour, for instance when it is invested in buildings, roads and public transport facilities. In the second place, the provision of many municipal services is contracted out. Real estate development is again a relevant example. Services in the domain of culture and recreation, which are labour intensive, will often be contracted out as well.

## 4 Conclusions and policy implications

Our empirical analysis shows that a reform that was designed partly to better compensate for disadvantageous socioeconomic composition fully capitalized into house prices. Only a small share of the reform-induced change in grants was passed on to residents through local taxes or spent on municipal staff. Hence, changes in grants appear to have been largely accommodated through an adjustment of the provision of public services that are valued by the marginal homebuyer, whereas our results are inconsistent with the presence of a bureaucratic flypaper effect.

The combination of limited income discretion at the local level and a dominant role of centrally assigned tasks may partly explain our findings, as it could result in underprovision of certain local public services. More local income and spending discretion, for instance through a larger reliance on local taxes rather than grants, may therefore enhance welfare.

The distributive effects of capitalization imply limitations on the use of fiscal equalization as an instrument for reducing real income disparities, as property owners are generally not the poorest inhabitants of municipalities that receive more grants.<sup>26</sup> However, in view of the popular resistance to nonmarginal differences in local public services in the Netherlands, the promotion of categorical equity is arguably a more important objective of fiscal equalization. Although the realization of this objective may appear to be less vulnerable to capitalization, the implication is still that improved local services come at the expense of a cut in private consumption for renters in the private sector and for homeowners who enter the market after the introduction of fiscal equalization – i.e. the majority in the long run. Hence, the promotion of categorical equity for these households is only legitimate to the extent that society values their consumption of public services more than the satisfaction of their other needs (Wyckoff, 1995). It is not obvious why this should be the case, though, since the politicians that determine local public service provision are selected by local voters.

The efficiency rationale for fiscal equalization is less affected by capitalization, as it does not depend on the distribution of benefits within municipalities. One source of inefficiency arises when local governments engage in redistributive policies.<sup>27</sup> Buchanan

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<sup>26</sup> See e.g., Oakland (1994) and Ladd and Yinger (1994) for more discussion.

<sup>27</sup> The literature provides various alternative efficiency grounds for fiscal equalization, relating for instance to fiscal externalities and rent sharing (Flatters *et al.*, 1974; Boadway and Flatters, 1982; Albouy, 2012) or interjurisdictional insurance (Persson and Tabellini, 1996a and 1996b; Bucovetsky, 1997; Lockwood, 1999). Allers (2012) argues that fiscal equalization enables proper yardstick competition, as fiscal disparities that are not transparent to voters bias interjurisdictional comparisons of public service levels and tax rates.



(1950) already noted that places with a positive ‘fiscal residuum’, for instance because of a large share of rich households, would attract too many households – see Boadway and Flatters (1982) for a formal analysis. This argument seems also relevant in the Dutch context, because of the substantial redistributive tasks assigned to municipalities.<sup>28</sup> For instance, without any equalization of spending needs, municipalities with a large share of households on welfare could hardly afford any other public services, thus inefficiently reducing the quality of life. The capitalization of an equalizing grant demonstrates its effectiveness in restoring the attractiveness of such places to the marginal homebuyer. Nevertheless, optimal equalization of spending needs and fiscal capacity may take a different shape when motivated by efficiency rather than equity considerations.

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<sup>28</sup> The decentralization of redistributive tasks is not unique for the Netherlands. For instance, Oates (1999) discusses the decentralization of welfare to the state level in the US. More fundamentally, it may be argued that almost any tax or expenditure decision of local governments has distributive implications (Boadway and Wildasin, 1984).

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TABLE 1  
Summary statistics (baseline regression sample)

Summary statistics (baseline regression sample)							
Variable	Obs.	Mean	Std. Dev.			Min	Max
			overall	between	within		
Panel data							
Real house price index	6704	214951	62705	49030	39158	66915	516630
Number of housing transactions	6704	241	363	343	120	1	3687
General grant per capita	6704	0.663	0.168	0.117	0.120	0.288	1.738
Reforms of 1997 and 2001 jointly	6704	-0.009	0.043	0.033	0.027	-0.271	0.237
Reform of 1997	6704	-0.009	0.043	0.034	0.026	-0.264	0.237
Reform of 2001	6704	0.000	0.012	0.007	0.010	-0.136	0.185
Transition grant 1997	6704	0.002	0.020	0.009	0.017	-0.116	0.483
Transition grant 2001	6704	0.000	0.008	0.004	0.007	-0.073	0.322
Reform of financing school buildings	6704	0.044	0.028	0.009	0.026	0.000	0.180
Transition grant for school buildings	6704	0.002	0.006	0.003	0.006	0.000	0.113
Property tax revenue per capita	5866	0.119	0.038	0.028	0.026	0.034	0.507
Municipal staff per 1000 inhabitants	5445	7.218	2.163	2.067	0.642	0.189	31.847
Cross-sectional data							
Permanent change 1997	419	-0.015	0.057			-0.264	0.142
Permanent change 2001	419	0.000	0.022			-0.136	0.081
Permanent change for school buildings	419	0.055	0.011			0.032	0.096
Number of inhabitants in 1997	419	37096	56937			4001	715148
Redemption of 1997 transition grants	419	0.003	0.026			0.000	0.346
Redemption of 2001 transition grants	419	0.001	0.012			0.000	0.232
Personal income in 2001	419	20.226	2.041			16.529	31.886

*Notes:* All grant amounts (including reforms and transition grants), personal income and the property tax revenue are expressed in 1000's of 2010 euros. Municipal staff is measured in full time equivalents. Reforms, permanent changes and transition grants are scaled to the population in 1997.

TABLE 2  
Correlations between reforms and income

	Permanent change 1997	Permanent change 2001	Permanent changes in 1997 and 2001 jointly
Permanent change in 2001	-0.221***		
	0.000		
Permanent changes in 1997 and 2001 jointly	0.927***	0.161***	
	0.000	0.001	
Log of personal income in 2001	-0.761***	0.366***	-0.630***
	0.000	0.000	0.000

*Notes:* Test statistics for deviation from zero are reported below each correlation coefficient.

Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

TABLE 3  
Baseline results

Panel A – Dependent variable: Log(real house price index)				
	OLS		IV	
	(1)	(2)	(3)	(4)
General grant per capita, 2 years lagged	0.0639* (0.0335)	0.106*** (0.0281)	0.292*** (0.0547)	0.333*** (0.0475)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes
R-squared	0.388	0.357		
Kleibergen-Paap F			698.0	593.6
Panel B – First stage corresponding to IV results				
Reforms of 1997 and 2001 jointly, 2 years lagged			0.959*** (0.0363)	1.182*** (0.0485)
R-squared			0.591	0.659

Notes: N = 6704, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

TABLE 4  
Identification on 1997 and 2001 reforms separately

Panel A – IV estimates. Dependent variable: Log(real house price index)				
	1997 reform		2001 reform	
	(1)	(2)	(3)	(4)
General grant per capita, 2 years lagged	0.345*** (0.0663)	0.353*** (0.0553)	-0.220 (0.192)	0.253*** (0.0789)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes
Kleibergen-Paap F	645.3	471.0	57.73	28.83
Panel B – First stage corresponding to IV results				
Reform of 1997, 2 years lagged	0.890*** (0.0350)	1.189*** (0.0548)		
Reform of 2001, 2 years lagged			0.652*** (0.0859)	1.455*** (0.271)
R-squared	0.591	0.646	0.460	0.658

Notes: N = 6704, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

TABLE 5		
Placebo: identification on reform of financing school buildings		
Panel A – IV-estimates. Dependent variable: Log(real house price index)		
	(1)	(2)
General grant per capita, 2 years lagged	0.0467 (0.0739)	-0.201 (0.127)
Municipality fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Municipality-specific time trends		Yes
Kleibergen-Paap F	102.4	102.7
Panel B – First stage corresponding to IV results		
Reform of financing school buildings, 2 years lagged	1.917*** (0.189)	1.148*** (0.113)
R-squared	0.513	0.650

Notes: N = 6704, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

TABLE 6				
The flypaper effect				
Panel A – Dependent variable: Property tax revenue per capita				
	OLS		IV	
	(1)	(2)	(3)	(4)
General grant per capita, 2 years lagged	-0.130*** (0.0241)	-0.0915*** (0.0219)	-0.236*** (0.0398)	-0.165*** (0.0301)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes
R-squared	0.173	0.0955		
Kleibergen-Paap F			884.7	681.7
Panel B – First stage corresponding to IV results				
Reforms of 1997 and 2001 jointly, 2 years lagged			0.887*** (0.0298)	1.150*** (0.0440)
R-squared			0.381	0.345

Notes: N = 5866, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

TABLE 7  
Impact of grants on municipal staff

Panel A – Dependent variable: Municipal staff per 1000 inhabitants				
	OLS		IV	
	(1)	(2)	(3)	(4)
General grant per capita, 2 years lagged	-0.450 (0.519)	0.161 (0.806)	-2.399** (1.078)	-0.0332 (0.821)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes
R-squared	0.0410	0.0410		
Kleibergen-Paap F			1050	751.0
Panel B – First stage corresponding to IV results				
Reforms of 1997 and 2001 jointly, 2 years lagged			0.877*** (0.0271)	1.130*** (0.0412)
R-squared			0.364	0.298

*Notes:* N = 5445, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.



APPENDIX TABLE 1  
Baseline results reproduced for real house price index in levels,  
demeaned for years and municipalities

	OLS		IV	
	(1)	(2)	(3)	(4)
General grant per capita, 2 years lagged	14439* (7830)	24259*** (6576)	68502*** (12850)	77624*** (11183)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes
R-squared	0.000452	0.101		
Kleibergen-Paap F			698.0	593.6

*Notes:* N = 6704, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

APPENDIX TABLE 2  
Results for alternative time lags

Number of lags $n$ :	0	1	2	3	4	5
Panel A – IV estimates. Dependent variable: Log(real house price index)						
General grant per capita, $n$ years lagged	0.151*** (0.0404)	0.276*** (0.0398)	0.333*** (0.0475)	0.331*** (0.0691)	0.229*** (0.0713)	0.105* (0.0611)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6704	6704	6704	6704	6285	5866
Kleibergen-Paap F	721.4	649.6	593.6	673.2	924.4	1498
Panel B – First stage corresponding to IV results						
Reforms of 1997 and 2001 jointly, $n$ years lagged	1.123*** (0.0418)	1.170*** (0.0459)	1.182*** (0.0485)	1.150*** (0.0443)	1.095*** (0.0360)	1.025*** (0.0265)
R-squared	0.416	0.568	0.659	0.736	0.750	0.768

*Notes:* Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**APPENDIX TABLE 3**  
**Alternative ways to deal with redemption of transition grants**

Panel A – IV estimates. Dependent variable: Log(real house price index)						
	Without municipalities receiving redemption		Redemption removed from transition grant		Redemption grant smoothed over time	
	(1)	(2)	(3)	(4)	(5)	(6)
General grant per capita, 2 years lagged	0.401*** (0.0645)	0.398*** (0.0593)	0.320*** (0.0558)	0.445*** (0.0468)	0.324*** (0.0578)	0.461*** (0.0549)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes		Yes
Observations	6320	6320	6704	6704	6704	6704
Kleibergen-Paap F	406.6	456.3	578.7	553.5	541.8	506.4
Panel B – First stage corresponding to IV results						
Reforms of 1997 and 2001 jointly, 2 years lagged	0.997*** (0.0495)	1.303*** (0.0610)	0.957*** (0.0398)	1.275*** (0.0542)	0.951*** (0.0409)	1.276*** (0.0567)
R-squared	0.590	0.654	0.591	0.659	0.590	0.659

*Notes:* Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**APPENDIX TABLE 4**  
**Identification on reform of financing school buildings, excluding transition grants**

Panel A – IV estimates. Dependent variable: Log(real house price index)		
	(1)	(2)
General grant per capita, 2 years lagged	0.129 (0.0794)	-0.178 (0.143)
Municipality fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Municipality-specific time trends		Yes
Kleibergen-Paap F	174.0	53.87
Panel B – First stage corresponding to IV results		
Reform of financing school buildings, excluding transition grants, 2 years lagged	3.053*** (0.231)	1.751*** (0.239)
R-squared	0.524	0.648

*Notes:* N = 6704, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

APPENDIX TABLE 5  
Miscellaneous robustness checks

Panel A – IV estimates. Dependent variable: Log(real house price index)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
General grant per capita, $n$ years lagged	0.330*** (0.0482)	0.333*** (0.0473)	0.346*** (0.0489)	0.325*** (0.0474)	0.285*** (0.0479)	0.227*** (0.0645)	0.205*** (0.0776)
Municip. fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6704	6704	6640	6160	6379	6704	6704
Kleibergen-Paap F	593.6	605.5	617.6	584.7	551.7	125.2	92.17
Panel B – First stage corresponding to IV results							
Reforms of 1997 and 2001 jointly, 2 years lagged	1.181*** (0.0485)	1.184*** (0.0481)	1.173*** (0.0472)	1.192*** (0.0493)	1.186*** (0.0505)	1.142*** (0.102)	1.195*** (0.125)
R-squared	0.659	0.659	0.722	0.672	0.638	0.652	0.651

*Notes:* Statistics are robust to heteroskedasticity and within-municipality correlation.

Significance levels: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

The columns of this table correspond to the following robustness checks:

- (1) No imputation of zero for missing observations for land lease in municipalities with less than 5% land lease;
- (2) Removal of transition grant for financing school buildings from general grant;
- (3) Four largest municipalities are removed from the sample;
- (4) Municipalities that received a bailout are removed from the sample;
- (5) Observations that are based on no more than 20 housing transactions are removed from the sample;
- (6) Observations are weighted by the average number of housing transactions per municipality;
- (7) Observations are weighted by the number of inhabitants in 1997.

APPENDIX TABLE 6  
Robustness checks for the flypaper effect

Panel A – IV estimates. Dependent variable: Property tax revenue per capita						
	1997 reform		2001 reform		Placebo	
	(1)	(2)	(3)	(4)	(5)	(6)
General grant per capita, 2 years lagged	-0.253*** (0.0485)	-0.181*** (0.0311)	-0.137** (0.0593)	-0.0690 (0.0535)	-0.0971*** (0.0361)	0.0123 (0.0343)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes		Yes
Kleibergen-Paap F	603.8	536.4	110.0	30.06	101.8	99.94
Panel B – First stage corresponding to IV results						
Reform of 1997, 2 years lagged	0.792*** (0.0322)	1.193*** (0.0515)				
Reform of 2001, 2 years lagged			0.804*** (0.0767)	1.261*** (0.230)		
Reform of financing school buildings, 2 years lagged					1.425*** (0.141)	1.071*** (0.107)
R-squared	0.377	0.311	0.166	0.341	0.242	0.324

Notes: N = 5866, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

APPENDIX TABLE 7  
Robustness checks for Impact of grants on municipal staff

Panel A – IV estimates. Dependent variable: Municipal staff per 1000 inhabitants						
	1997 reform		2001 reform		Placebo	
	(1)	(2)	(3)	(4)	(5)	(6)
General grant per capita, 2 years lagged	-1.927 (1.307)	-0.0784 (0.930)	-4.419* (2.340)	0.258 (0.888)	-1.938 (1.632)	0.0531 (1.009)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes		Yes
Kleibergen-Paap F	572.2	633.4	132.8	33.65	101.6	90.43
Panel B – First stage corresponding to IV results						
Reform of 1997, 2 years lagged	0.768*** (0.0321)	1.171*** (0.0465)				
Reform of 2001, 2 years lagged			0.860*** (0.0746)	1.192*** (0.206)		
Reform of financing school buildings, 2 years lagged					1.077*** (0.107)	1.025*** (0.108)
R-squared	0.359	0.256	0.123	0.274	0.195	0.260

Notes: N = 5445, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

FIGURE 1  
Reform-induced change in grants scattered against personal income

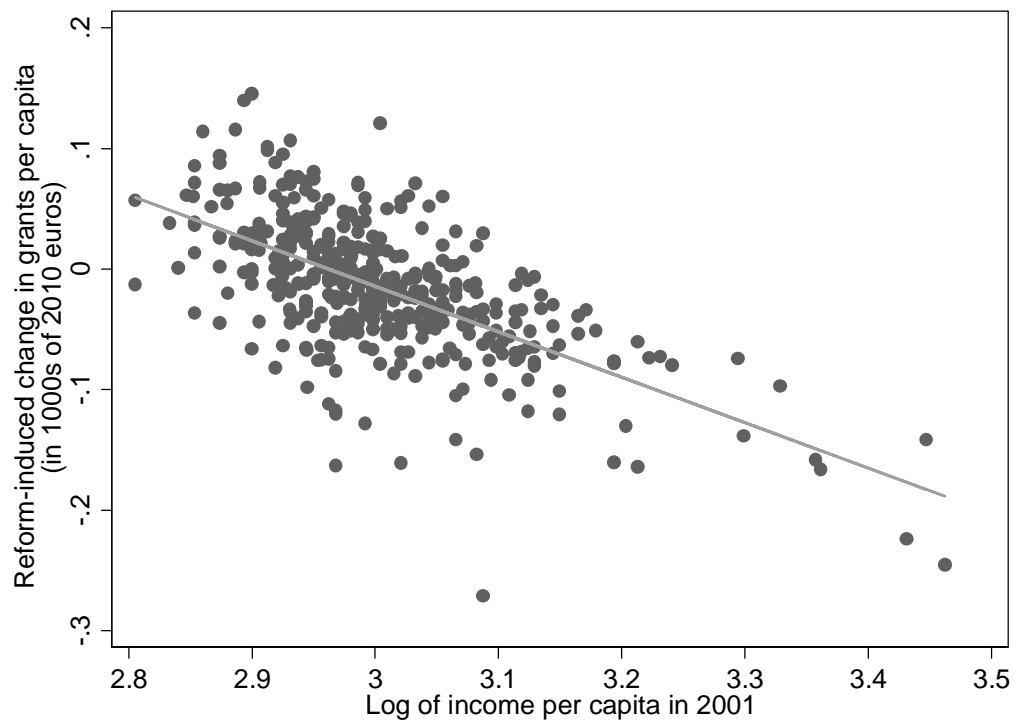


FIGURE 2  
Permanent change in 2001 scattered against permanent change in 1997

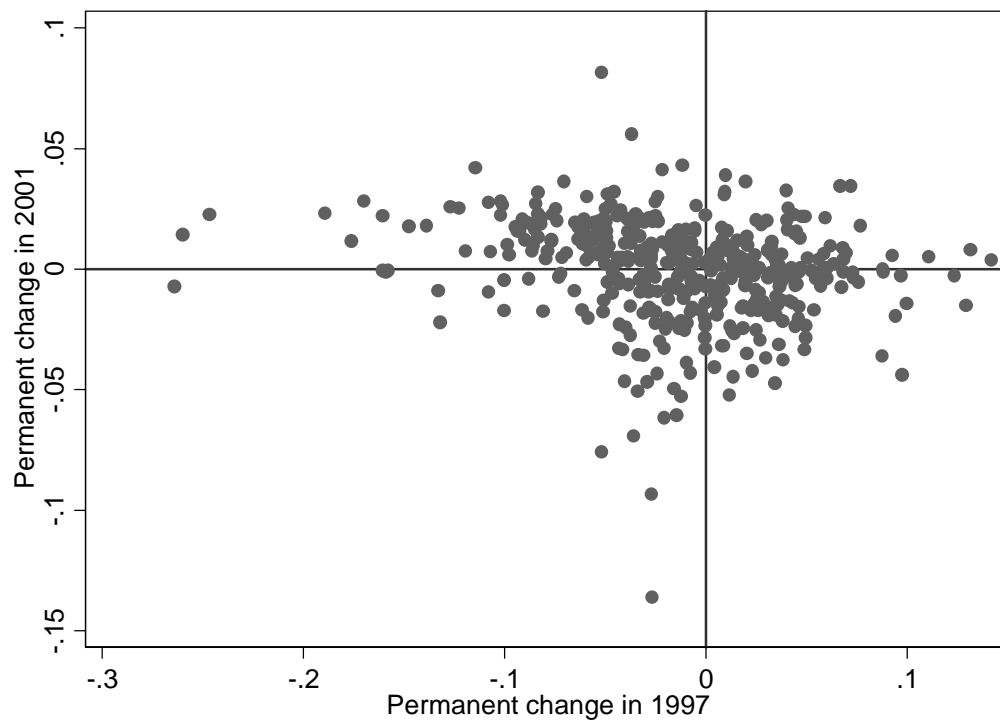


FIGURE 3

Reforms of 1997 and 2001 separately for the municipality of Amsterdam

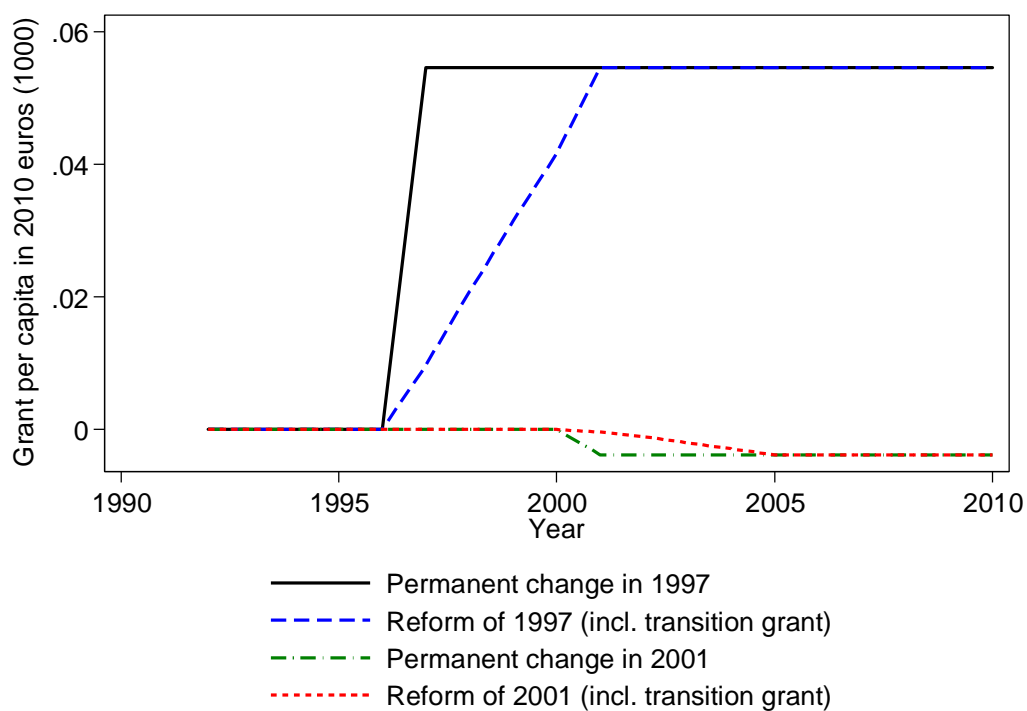


FIGURE 4

Reforms of 1997 and 2001 jointly for four municipalities

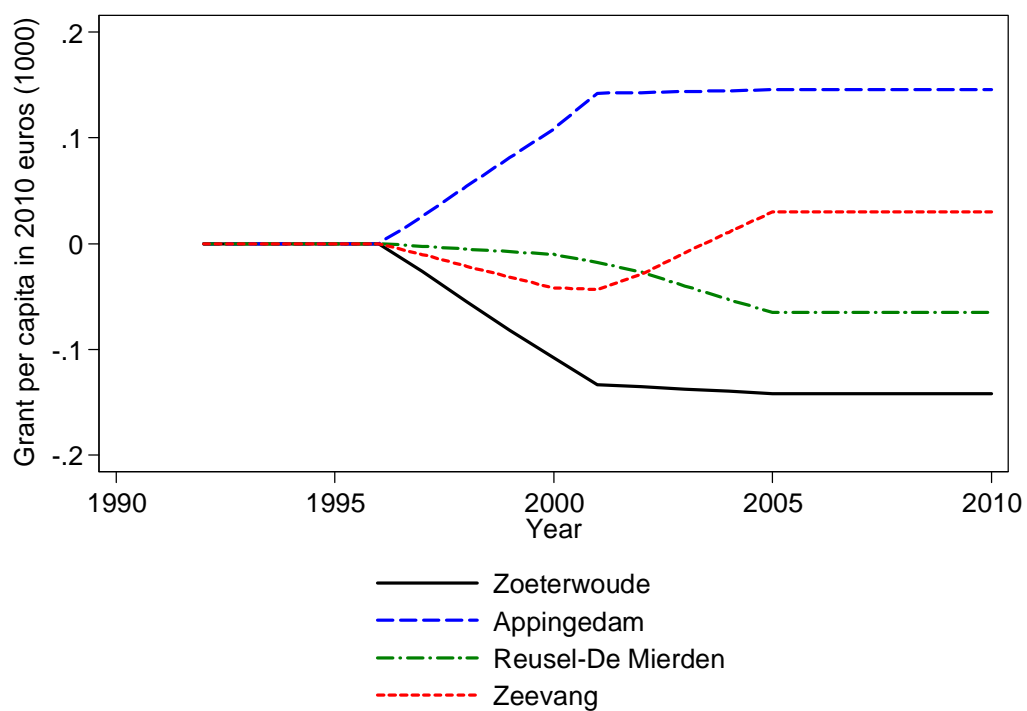


FIGURE 5  
Reform of 1997 for municipality of Bloemendaal

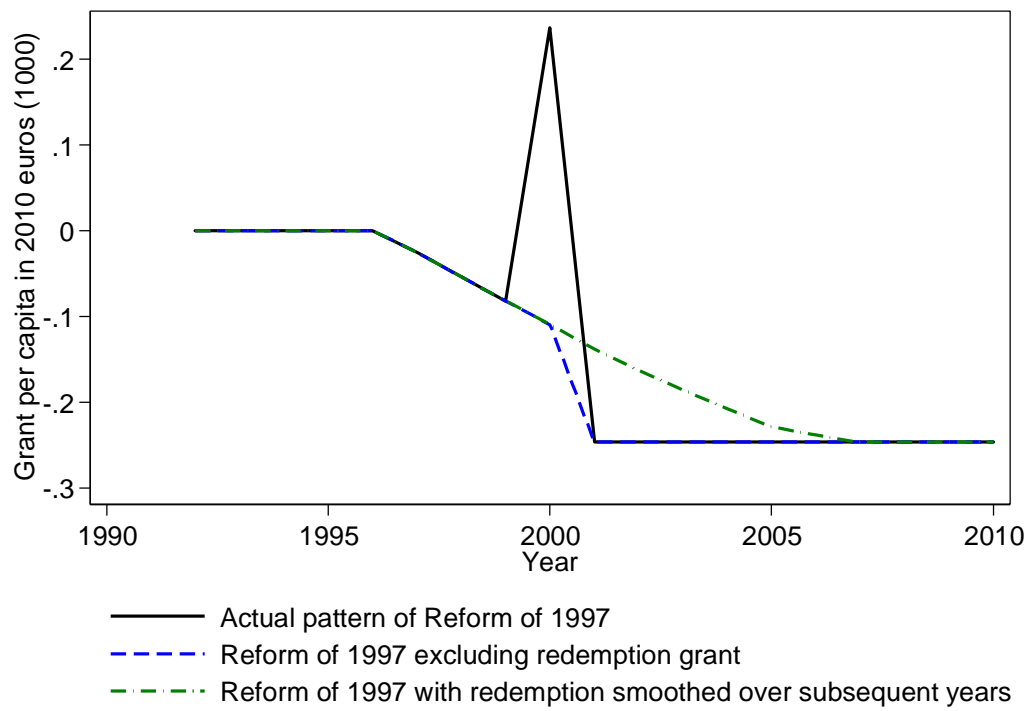
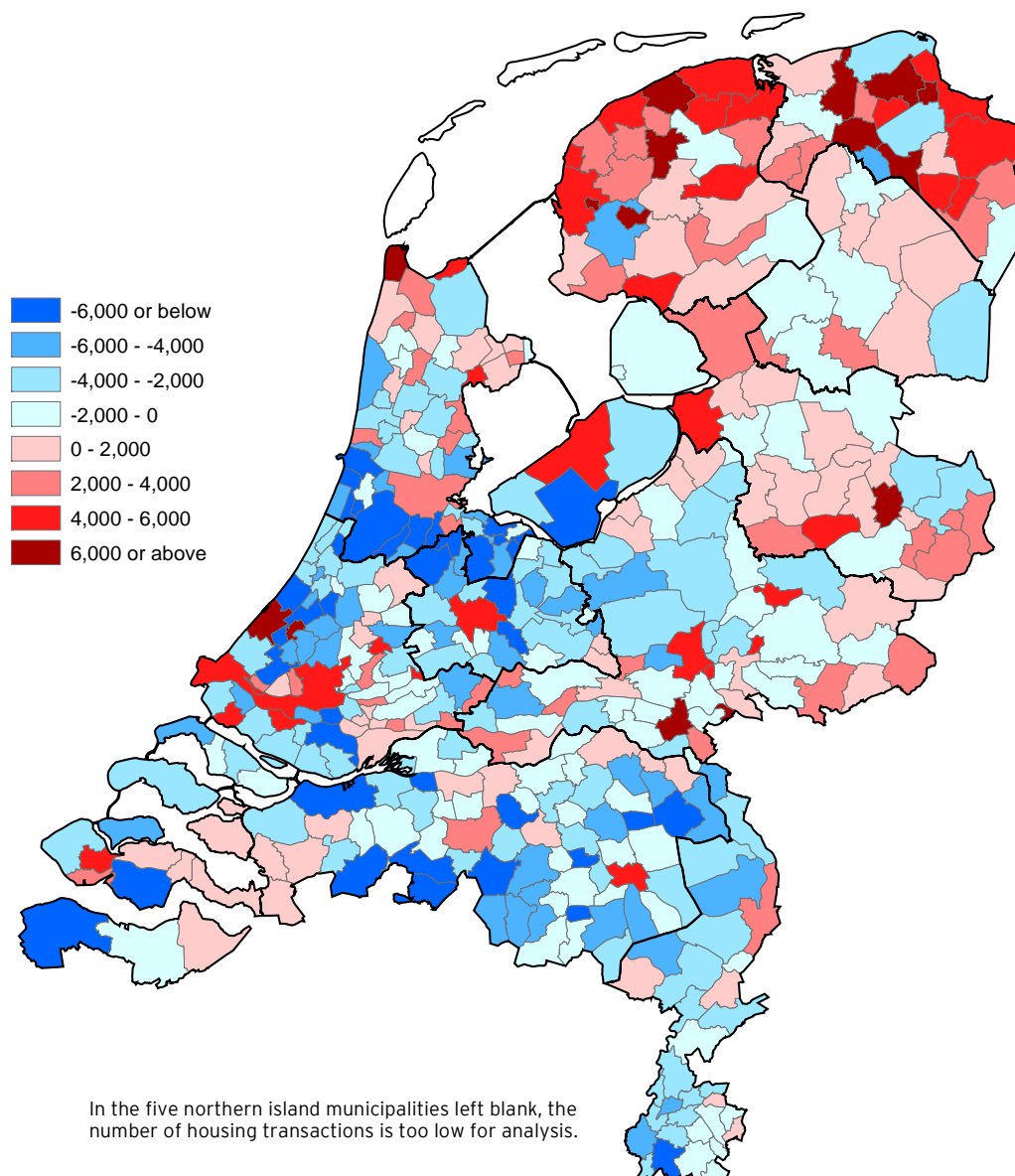


FIGURE 6  
Estimated effect on house prices of 1997 and 2001 reforms jointly (in 2010 euros)





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